THE MEASUREMENT OF CONCENTRATION OF WEALTH

THIS brief paper is intended primarily as a reply to an article of Dr. W. E. Persons in the May number of the Quarterly Journal of Economics; but it contains also some further development of the points under discussion. The reader would do well to have that article before him. Since the question at issue is chiefly a matter of method in the narrower sense, I will, like Dr. Persons, take the figures used at their face, and will avoid entering upon any consideration of their reliability and comparability. It is best to deal here with but the one question as to what is a correct measure of concentration.

I. *The " Coefficient of Variability "*

The " coefficient of variability " used by Dr. Persons is a function of the so-called standard deviation. This concept relates to the theory of " error " and to the probability curve. Dr. Persons refers to Professor Karl Pearson in this connection. The " coefficient of variation " is defined by Professor H. L. Moore and used by him in a study of the Variability of Wages,[1] as is mentioned by Dr. Persons. It is obtained for any set of figures by dividing the arithmetical average into the standard deviation as calculated from that average. This process makes the test of " variability " or concentration thoroly relative, as it should be.

Dr. Persons says[2]: " The statistical problem before the economist in determining upon a measure of

[1] Pol. Sci. Qu., vol. xxii, esp. p. 64. [2] p. 431.

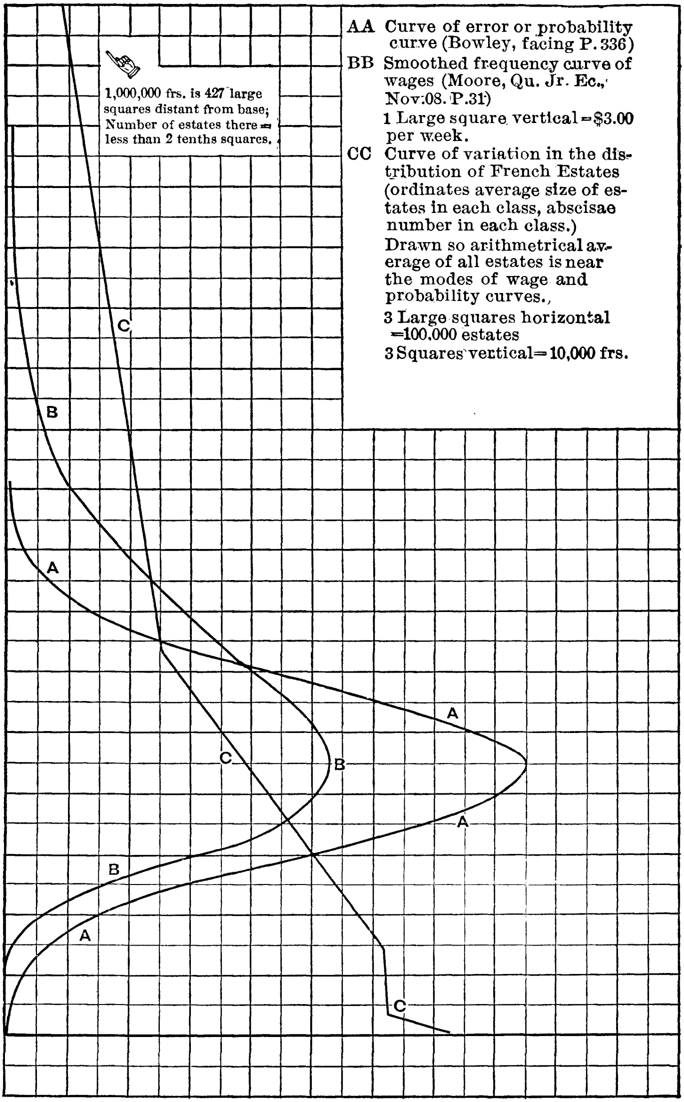
the inequality in the distribution of wealth is identical with that of the biologist in determining upon a measure of the inequality in the distribution of any physical characteristic. In order to determine the variability of the stature of men in a certain community, the investigator would need a statistical table giving the number of individuals of the varying heights. . . . In order to determine the variability of the income or wealth of men of any community, such statistics would be substituted for those of stature."

I find, on the contrary, that the statistical problem before us is evidently *not* "identical" with the problem of variation as studied by the biologist.

A common illustration of the mode of distribution of variants about a mean is the case of a marksman's shots as scattered about the bull's eye. If there is no cause of constant error, these shots will be most dense at the centre of the target and more sparsely distributed as the distance from the centre increases. If we had no means of knowing the point aimed at other than the distribution of the shots, we might determine it as that point from which the deviation of all the other shots taken together is minimal. The method of least squares is used to get the "most probable" [1] mean of several measurements differing from each other owing to causes analogous to those which send the marksman's shot one or the other side of the bull's-eye. Dr. Persons mentions the familiar case of astronomical observations. But he does not stop to consider whether the curve of the distribution of wealth has the symmetrical bell-shaped character of the probability curve. It is obvious, however, that the number of estates or of private fortunes is not greatest

[1] "Most probable" is here used in the technical sense of according to the theory of probability or the law of error.

DIAGRAM I


1,000,000 frs. is 427 large squares distant from base; Number of estates there = less than 2 tenths squares.
AA Curve of error or probability curve (Bowley, facing P. 336)
BB Smoothed frequency curve of wages (Moore, Qu. Jr. Ec., Nov. 08. P. 31)
1 Large square vertical = $3.00 per week.
CC Curve of variation in the distribution of French Estates (ordinates average size of estates in each class, abscisae number in each class.)
Drawn so arithmetrical average of all estates is near the modes of wage and probability curves.
3 Large squares horizontal = 100,000 estates
3 Squares vertical = 10,000 frs.
A
B
C

near the average, tapering away from it in both directions. On the contrary the number of estates within a given range is uniformly greater as the size of the estates is smaller. The curve of the "variation" of the size of estates, that of the variation of wages, and the probability curve, are plotted in the accompanying diagram in a way to facilitate comparison. The curve of the variation of the size of estates should not be confused with the curve of the distribution of the property in those estates. The abscissae of the latter represent the number of estates of a given size or larger, not the number in a particular class.

It is obvious that "nature" (or Providence) is not aiming at making all men equal in respect of property. The "natural" condition, on the contrary, seems to be one of inequality. There is no *typical* size of estate or fortune. The arithmetical average, the mode, and the median do not tend to coincide, as they do in the case of the markman's shots and of any phenomena distributing themselves in general conformity to the "law of error" which finds expression in the probability curve. That which is a proper measure of the dispersion of shots about a bull's eye, or of the variation of stature about a mean, or of the variability of wages, — where, also, the mean is something more than the sum of the items divided by their number — is not as a matter of course applicable to the measurement of concentration of wealth. Dr. Persons' application of the standard deviation to test concentration of wealth derives no support from a consideration of the previous uses or the purposes of the method of least squares.[1] The distribution of wealth is not analogous to the "variations" of biology.

[1] It is worth mentioning, tho a minor point, that Dr. Persons' standard deviation is not correctly obtained. By treating all estates in a class as if they were of the size

II. *Whence should Variation be Measured and how are Variants to be Weighted?*

The "centre" chosen by Dr. Persons from which to measure variability is the arithmetical average. In the perfect symmetrical probability curve the arithmetical average, the median, and the mode coincide, In the curve of the distribution of wealth and in that of the "variation" of the size of estates they part company entirely. Hence the choice between these may mean important differences in the results obtained.

Dr. Persons chooses the arithmetical average on practical rather than scientific grounds. He says: "The deviations are taken from the arithmetic average, which is perfectly definite and easy to compute. In the writer's mind the arithmetic average best represents the 'plane of distribution.' If the median were used, some theory of distribution would have to be assumed in order to estimate it from the frequency table." [1]

If there is a "plane of distribution" for estates, as shown *e. g.* in the curve of Diagram I, it is not obvious to me. The mode may ordinarily be taken to be the most "representative" average. But in this case the mode is scarcely above zero. It would appear, therefore, that the point from which "variability in the distribution of wealth" should be measured is zero, or so little above that as to make it best to measure variation from the zero point. The median is, next

of the arithmetical average of their class he does just that which he would avoid in his choice of the arithmetical average as the centre from which to measure deviations; that is, he assumes a law for the distribution of estates between the limits of a class and one that does not fit the facts. (See p. 165.) Such procedure is correct for getting an *average* error, but not for the standard deviation, tho the falling short of correctness may be of little practical importance.

[1] p. 448–9.

to the mode, the most "representative" average. That the arithmetical average is influenced by the extreme cases in full proportion to their extremeness would, in my judgment, make it less representative. Ought the very large fortunes, or any addition to them that leaves other fortunes as they were before, to be weighed in a balance whose centre is determined so largely by their own distance from it? By making the arithmetical average larger, they increase the size of the divisor and tend to lower the coefficient. The arithmetical average is scarcely adequate to the use Dr. Persons makes of it.

There is another disadvantage of the arithmetical average. It will be greatly influenced by the arbitrary inclusion or exclusion of estates near zero, where there is bound to be much uncertainty and inaccuracy.

Dr. Persons gives no reason for using the standard deviation instead of the average error as a test of concentration. The conformity of the statistics to the law of error would be a good reason. But they do not conform. It would seem to be natural to give the average error a first trial, supposing we can find a proper "plane of distribution" from which to measure the deviations.

One important effect of the use of the standard deviation Dr. Persons does mention. He says: "The larger and smaller estates are automatically weighted by the use of the squares of the deviations. No other more satisfactory method of weighting has been suggested."[1] I can think of no good reason for giving the extreme cases any other than their natural weight, *i. e.*, let each estate count according to its amount.

[1] p. 448.

III. *The Coefficient of Variability as Applied by Dr. Persons to Incomplete Data*

Even if the statistics to be tested by the coefficient of variability are representative and complete, the applicability of this criterion is evidently open to question. But suppose they are not representative or complete, but are instead partial and fragmentary?

Dr. Persons of necessity deals with statistics where those of estates of smaller size are not merely inadequately or incorrectly returned, — they are often altogether wanting, as *e. g.* in the case of the net values of British estates under £100. As if to make the case against him perfect, he has calculated his coefficient for statistics that are less and less adequate, as he omits from consideration more and more of the smaller estates.[1] Under this treatment, his coefficient varies extraordinarily for the same figures.[2]

Dr. Persons' treatment of the statistics in effect assumes that it is not significant if you truncate the area subtending the curve of distribution and representing the volume of riches. The effect of this can be represented graphically as in Diagram II, Figure 1.

It is evident at a glance that, taken by themselves, the ordinates to the left of B D will exhibit less variation or deviation from their average height than is the "variation" of all the ordinates from A E to C. Not only will the variation be less, but the average height of these is greater than that of all the ordinates. Hence the dividend being made smaller, and the divisor larger, by truncation, the resulting ratio, that is, the "coefficient of variability" tends to be artificially reduced by the use of the incomplete figures. To get the full

[1] Cf. the last column of Table VII, p. 444. [2] Cf. p. 441.

effect of the failure to take account of the progressive omission of more and more small estates, one should suppose the truncating ordinate B D moved farther and farther to the left. One will see how, other things equal, the range and variation of the cases must rapidly decrease *relatively* to the mean distance from the 0 abscissa. How the fact that other things are not equal, specifically how the greater steepness of the curve in its upper portions affects the situation, we shall see presently.

DIAGRAM II

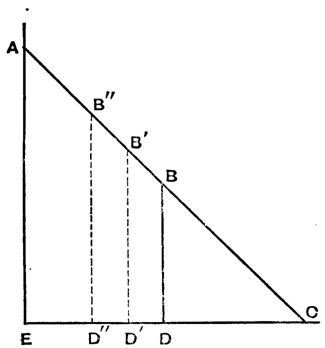


Fig. 1

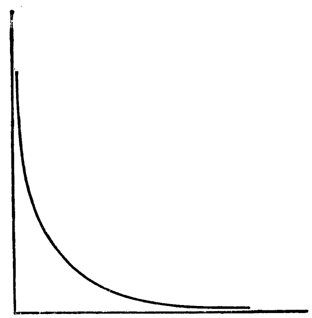


Fig. 2

Dr. Persons might have forestalled such an objection by taking into consideration only that part of the area subtended by the curve that lies above a horizontal line passing through B. Then his average would be that of estates above B less B D, and his "variability" would not be artificially reduced by the size of his divisor. The new base line becomes the line of reference for everything. If it appears somewhat artificial, so are the truncated statistics. Dr. Persons' coefficient would appear to be entirely satisfactory if he would only take as his divisor the average *less* the smallest estate in his group.

But this is entirely true only *if* the curve showing the distribution of riches as drawn arithmetically be a straight line. We know it is not a straight line. It is in fact highly concave. Instead of resembling the curve of Fig. 1 it is approximately a rectangular hyperbola, which means that it is concave at every point in its course and well pushed in towards the ordinate and abscissa of reference. It has the form roughly shown in Fig. 2. What effect does this fact have upon Dr. Persons' application of his coefficient to incomplete data?

If the average used in getting the coefficient be the reduced average suggested above, should we not expect the coefficient to vary greatly for the same body of statistics according to what classes of estates are omitted? Since we cannot satisfactorily determine the point of truncation for two bodies of statistics by merely making it arithmetically the same for both (of course first reducing monetary units to the same standard), without ignoring the factor of relativity, it is difficult to perceive any practical way of meeting this objection, though it could be met theoretically by cutting the curves to be compared at corresponding points. Even if it be considered sufficient for practical purposes to be guided by the absolute size of estates, it is interesting to note how seldom the division lines for French and British estates are the same — once only, *i. e.* at £10,000 and 250,000 francs — and then the boundaries have different meanings.

Approximate equality in the distribution of property would make the arithmetical curves of distribution (such as those of Diagram II) highly convex instead of concave. The actual curve is highly concave, and concave throughout its length. But cutting off the flattened portion at either end would make it less so.

Hence a test of concentration that treats incomplete figures as if complete should show somewhat less than the actual inequality.

A method, which is open to criticism as not appropriate to Dr. Persons's material when applied to complete statistics (if there be any such), is made doubly and trebly objectionable as he actually applies it, to material that does not profess to be complete, and that is made less so by his manner of treating it.

IV. *The Supposed Superiority of "Some Numerical Measure"*

Dr. Persons evidently attaches importance to getting some numerical measure of concentration. He gives this characteristic a prominent place among the advantages of his coefficient. He makes it the basis of a general criticism of graphic methods, tho the following is specifically directed against the logarithmic curve: "No numerical measure is offered. If, in comparing two curves, the second is steeper at one point than the first, and not so steep at another point, there is no way of telling the net result." [1]

Dr. Persons seems to magnify the difficulty of judging slants. He says, "the comparison of slopes of the curves has to be made entirely by the eye." The differences disclosed in the curves that are used in the article criticised are so great that this is not an actual difficulty. If they were not great enough to be seen easily, the correct judgment would be one of approximate identity in the degree of inequality. There are pretty sure to be guide lines on a diagram which aid the eye, and if not, there is no difficulty in comparing the distances separating two curves at various points

[1] pp. 448, 428.

by the use of a ruler. The " net result," also, is indicated by the total divergence of the curves.

Just before the above remark is the statement: " The exact meaning of the test is not evident. As the author himself says: ' Just what the degree of difference is between two series of numbers so compared is not obvious '." Dr. Persons fails to bring out the full meaning here by omitting the rest of the passage, thus: " at any rate to one who is not a trained mathematician, but the *direction* of the difference is unmistakable." " Direction " is in italics in the original and the intention to contrast " degree " and " direction " is perfectly clear. The *direction* of the difference is important for comparison. But I did not feel able, and did not attempt, to measure the divergence of the curves compared in any way such that one curve might be said to exhibit a quantitatively determinable degree of concentration, say twice as much as another. We can compare curves but cannot, with safety, compare the differences between them in this way.

Is this situation improved by the use of a numerical test? It is perfectly obvious that we can apply our arithmetic and say that one coefficient is twice as great as another. Thus one might be disposed to say, on the basis of Dr. Persons' coefficients,[1] that concentration for all successions in France is over twice as great (1620 is the coefficient) as for those of 10,000 francs (709), and this again over twice as great as for those over 250,000 francs (279). Many would doubtless take such figures to mean that concentration is " twice as great " in the one case as in the other. There is no warrant for this kind of arithmetical manipulation. The fact that it is likely to occur seems to be an objection to the use of *any* numerical measure of concentra-

[1] p. 444.

tion, at least until the mathematics of its measurement are further developed than at present. Dr. Persons himself confines his conclusions to "greater" and "less," thus discussing only direction, and *not degree*, of difference in concentration.

As regards the possibility of a difference in the slants or slopes of the logarithmic curves at different parts, it is one of the distinct advantages of a graphic method that these are preserved. If the "net result" is also in evidence, a graphic would appear to have distinct advantages over a numerical test, only the trouble of making the drawing being in the other pan of the scale.

V. *The Lorenz Method as Arbiter*

To decide the issue, as regards comparative concentration in Great Britain and France, between the indication of the logarithmic curves and that of his own coefficient, Dr. Persons calls in Prof. Lorenz's test — a graphic method — as arbiter. He procures a verdict in his own favor. It is surprising that he fails to note the other case discussed by him, where the verdict, under the same arbitration, is two to one against his coefficient. He says [1] that the Lorenz method shows least concentration at the earliest of the four Massachusetts periods compared. This agrees with the test by logarithmic curves. But later [2] he concludes from coefficients: "Probates in Massachusetts showed the greatest concentration in 1879–81 and the *least concentration in 1889–91*." (Italics are mine.) It is necessary, therefore, to examine Prof. Lorenz's method.

I have no theoretical ground of objection to the Lorenz test, provided the statistics are complete. But

[1] p. 417–418. [2] p. 447.

it is doubtful whether they ever are nearly enough complete to meet the requirements. Even supposing that, in the use of this method, it is permissible (as it is inevitable) to leave out the propertyless class, the line drawn between the propertyless and the small-propertied is bound to be very uncertain and unreliable. But a small difference in method of compilation at this point will affect *every part* of the Lorenz curve. Of the statistics brought into the present controversy, moreover, only the French successions meet the primary requirements of the method. The British net figures extend down only to £100 and at that point are perhaps of doubtful accuracy. The Massachusetts probate statistics are not net, and it is doubtless the small estates that are most affected by undeducted debts.

What if the Lorenz method is applied to truncated statistics? Since the curve of distribution is approximately an hyperbola, always getting steeper, the truncation is bound to be, for any test which calls for complete data, of some effect upon apparent concentration. Just what the effect may be, I will have to leave to the mathematicians.

The Lorenz test, also, like that of Dr. Persons, must be affected by the varying distance between the lower limit of the statistics used and the zero fortune. This effect could be obviated by subtractions, like those mentioned above in the discussion of the proposed coefficient.

If we are to cut off the lower portions of the areas representing the amount of riches, how can it be so done as fairly to compare the two sets of statistics? The proper point of truncation cannot be obtained by considering merely the absolute numbers, though there are difficulties even in the way of this simple procedure.

If the test is to be properly relative, per capita wealth must be taken into consideration. If the truncation is not at the same *relative* point in each curve, one curve becomes factitiously steeper than the other.

The differences in the sizes of the British and French estates, as well as the incompleteness of the statistics, and the fact that the lower limits are not the same in terms of absolute amount and presumably not relatively, deprive the Lorenz test of conclusiveness. Just how the result is affected I cannot say. The application of the same test to the Massachusetts statistics, on the other hand, is less open to objection, since they are complete in their way. It would seem, therefore, that the verdict of the Lorenz method favors the logarithmic curve rather than the proposed coefficient.

Dr. Persons makes a good suggestion for overcoming the practical objection to the Lorenz method.[1] But one is curious to know why he connects the points by straight lines in one of these diagrams and by curves in the other. It may be because of the sparsity of the determining points in the second diagram. This suggests an advantage, minor it is true, but worth mentioning, of the logarithmic curve. We do know that the line connecting the determinate points in that case is approximately a straight line. In the Lorenz curve we only know that it is *not* straight, and the points may be so far apart as to leave too much to the discretion of the draughtsman.

[1] pp. 441–442.

VI. *The Advantages of the Logarithmic Curve in General, and also as Modified by the Use of the Average Instead of the Least Size of Estates in Each Class*

Dr. Persons mentions my departure from Pareto's similar use of logarithmic curves, namely, in plotting numbers of estates along the horizontal axis (where Pareto employs the vertical) and size of estates along the vertical axis. The departure was not conscious and also not strange, since I was under no direct obligation to Pareto for the method. The choice was due to the fact that I had in mind the comparison of the "pyramid of fortunes" with the pyramid of ages. In the curve the "pyramid" is put all on one side of the vertical axis. But the conception of the distribution of riches as pyramidal is helpful, even though the curve used to represent it does violence to the pyramid mechanically.

The straightness of the logarithmic curve is, as Dr. Persons says, not essential to the use I make of it. But the fact that it is approximately a straight line has its advantages, as above mentioned, in relation to simplicity and accuracy of construction. Where the number of points definitely determined is comparatively few, it is more than convenient to have an empirical law for interpolation which lends itself so readily to draughtsmanship.

The great advantage of the logarithmic curve, however, relates to the fact that it can be applied without loss of correctness to incomplete data. The omission or the unreliability of the statistics of small estates *does not affect the curve for the remainder.* It is just as if we were compelled to disregard all except the upper portion of an ordinary curve. If so much is reliable

we can make the necessary allowances for distortion along the lower portion, or, for some purposes, disregard that part altogether. The factors which determine the form of the most trustworthy portion of the curve are probably operative throughout the range of distribution.

The adaptability of the logarithmic curve is quite in contrast with both the Lorenz and Persons tests. These require a complete record for use to good advantage. Dr. Persons says:[1] "Objection may be offered to the coefficient of variability because tolerably complete statistics are necessary in order that it may be used. Answer is to be given that only when we do have complete statistics is *any* comparison of variability to be depended upon." The opinion of statisticians would surely not sustain Dr. Persons' implied judgment — which is also contrary to his own practice — that we should wait for complete data before drawing a conclusion. The most that is necessary is due caution in affirming an opinion based on data that are not representative. Any numerical data accurately described, and not cooked or concocted with extraordinary cleverness, may be of use to the statistician.

One important improvement upon the logarithmic method as I have used it does occur to me as a result of my thoughts having been turned towards the subject again. I have plotted as vertical ordinates the *lower limit* of classes of estates by size. It would be better to use for these ordinates the *average* size of estates for each class. This would remove an objection of Prof. Lorenz against various methods, repeated by Dr. Persons in relation to the logarithmic curves.[2] The size of estates in the uppermost class is not disregarded if the averages, instead of the lower limits, are

[1] p. 449. [2] p. 428.

plotted. This is the only theoretical advantage of the modification and it affects the least important part of the statistics.

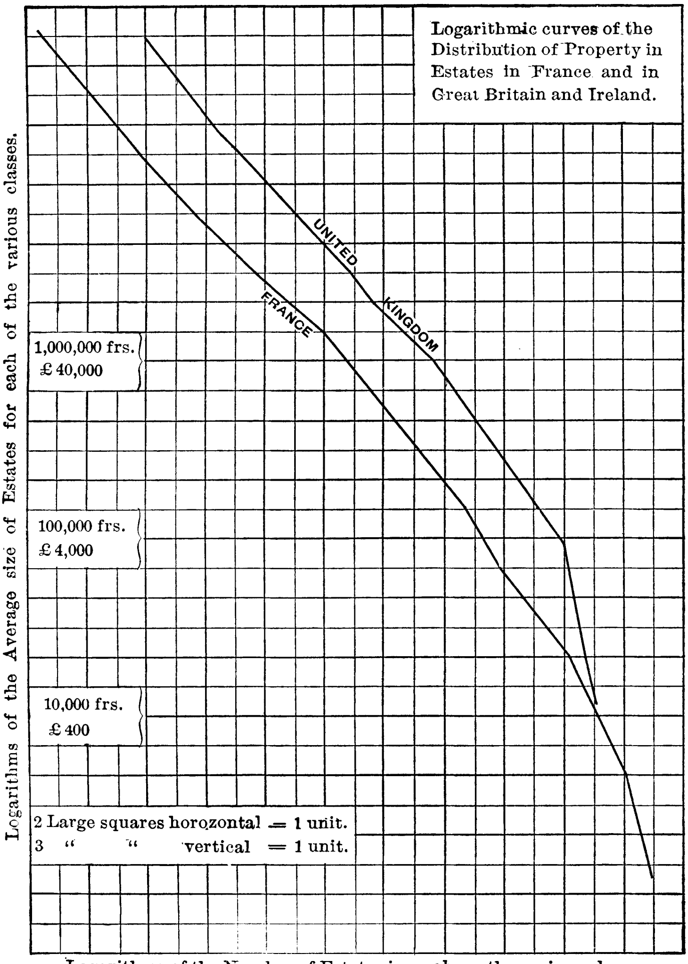
The number of estates exceeding any given amount ought to be sufficient to determine that point of the curve. But in fact boundary lines between classes are often points where taxes change, hence the number of estates is likely to be distorted just here. Using the average reduces this distortion, since the carelessness about reducing estates clearly above a certain limit may counteract carefullness to lessen the figure for those just above it. Whether this lower limit is made inclusive (France) or exclusive (Great Britain), also, is by the use of the average size made an indifferent matter.

In Diagram III are logarithmic curves plotted on the basis of numbers and average amounts of estates for France and the United Kingdom. Owing doubtless to the differences in the method of compiling the statistics in the two countries, the curves diverge less than the logarithmic curves previously drawn without the refinement of averages. This helps us to understand how the inadequate, but not in all respects wrong, tests of Dr. Persons yield a qualitatively different result. According to these curves of Diagram III, it is clear that Great Britain shows greater, though only slightly greater, concentration than France. (The lateral compression of the scale to fit the page reduces the apparent divergence of the curves.) At the 100,000 franc point the two curves are 12½ tenth's (large) squares apart; near the upper ends, 17. Less importance is to be attached to the abrupt lower end of the British curve. In making this comparison in the article which Dr. Persons criticises, I have called attention [1] to the

[1] See the footnote, p. 48.

fact that *inter vivos* gifts are included in the British statistics while excluded from the French. This fact

DIAGRAM III



strengthens the conclusion drawn as to greater concentration in the United Kingdom, and it must corre-

spondingly weaken or destroy the force of any inference from the comparison of these statistics pointing to the opposite conclusion, for the inclusion of *inter vivos* gifts must reduce the apparent concentration of the British statistics.

VII. Conclusion

All the objections raised by Dr. Persons to the use of logarithmic curves, as enumerated by him,[1] are met in the foregoing discussion. The meaning of the test *is* evident to one who is familiar with curves and with the significance of logarithms. The eye is put to no severe strain to tell whether two lines whose position may be shifted right or left at will are parallel or not. The modification of the curve by the use of averages for the size of estates meets the objection with regard to the largest estates. The objection, however, is not in itself very effective, since it assumes that a cause of concentration which has statistical importance can affect only a few estates and will not change the relative numbers in the classes.[2] The smaller estates were not plotted for reasons that appear above or are evident in the character of the statistics compared. After correcting the misprinted "83 per cent," to 29, the variation in the proportion of the total number of probates that lies below the absolute amount $1000 for the various periods of the Massachusetts statistics is, with the exception of the decline in the percentage from 1880 to 1890, what should be expected to result from the increase in per capita wealth.

Certain positive claims of Dr. Persons for his own method are enumerated.[3] The necessity of having

[1] At p. 428 of his article.

[2] The point is discussed in a footnote at page 37 of the article in the Publications of the American Statistical Association.

[3] p. 448 of his article.

complete statistics is a limitation, not an advantage, of his method; or if the first point refers to each estate having its effect on the coefficient, that is not peculiar or remarkable. It is equally true of the modified logarithmic curve. The weighting is unexplained and is a mistake. There is no "plane of distribution," but the rest that is said of the arithmetic average is sound. The question of computation is of no importance for present purposes.

It is well, with regard to the consistency of all the evidence, that Dr. Persons' conclusions do not stand. There is so much collateral evidence of greater concentration in Great Britain than in France that a settlement of this particular question of interpretation in his favor would rather open than dispose of the general questions involved. This applies also with reference to the tendency to concentration in the United States. It is not credible that the concentration of riches in Massachusetts, with all its urbanism and industrialism intensified, was less in 1890 than in 1830.

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The numbers in the following refer to the sections of Dr. Watkins's Note

I

Dr. Watkins objects to the use of the coefficient of variability in measuring inequality in the distribution of wealth or income because the frequency curves in such cases are not the normal curves of distribution for which the standard deviation and coefficient of variability were originally derived. I had no idea that any one would suppose the application of the coefficient of variability to wealth and income statistics to be made on the ground of any supposed similarity between wealth-frequency curves and the normal curve of distribution. My argument for the adoption of the coefficient of variability was not based upon *a priori* reasons. On the other hand, the function $\frac{\sigma}{m}.100$ was applied to hypothetical and actual statistics in order to determine *how it would work*. Exactly the same kind of argument was used by Pareto and Dr. Watkins in justifying the methods that they advocate. I pointed out the character of my argument and its justification in Section VII of my article. No assumption was made that " 'nature' (or Providence) " is aiming to make all men equal in respect of property. In saying that the " statistical problem " of the economist and of the biologist is identical I meant to draw the parallel that both economist and biologist have to deal with series of individual measurements, the items of both sets of series vary among themselves, and the statistical problem in both cases is to obtain a measure of that variation. The measure that biologists use was therefore applied to wealth and income statistics in order to analyze the meaning and effect of that application experimentally.

II

I stated that "the arithmetic average best represents the 'plane of distribution,'" and chose such average as the point from which to measure deviations. If the national income in 1850 was such as to give an arithmetic average income of $1000, and in 1900 such as to give an average of $2000, then, according to my notion, $1000 and $2000 fix the two "planes of distribution" from which to figure the respective deviations.

The point from which to measure dispersion is not dictated by economics or mathematics, but by current habits of thought. We desire to answer the query, is wealth (or income) being distributed more or less *unequally?* Although writers on economics have not defined "increasing concentration of wealth" in mathematical terms, it is clear that they think of it as meaning a movement away from equality in distribution. In speaking of the distribution of wealth Seager uses the word "unequal." [1] Gide speaks of the "inequality in distribution," [2] and illustrates his concept by imagining the total wealth of France to be divided into equal portions.[3] Ely says, "In general terms we may say that it (the concentration in the ownership of wealth) means a divergence from an equal distribution, — when wealth is equally distributed there is no concentration, when it is all in the hands of one person there is the greatest possible concentration." [4] Edwin Cannan says, "To the ordinary person who has not been infected by the study of economic text-books, the term 'distribution of wealth' has a very definite, intelligible, and useful meaning.

[1] Introduction to Eonomics, p. 593.
[2] Political Economy (Jacobson's translation), p. 401.
[3] Ibid., p. 407. [4] Evolution of Industrial Society, p. 256.

— An 'equal distribution' means an equal division: a 'change in distribution' means a change in the proportions in which the total is divided."[1] In the *Growth of Large Fortunes* Dr. Watkins discusses the question, "Is there undergoing — a process of concentration into the hands of a few, and are sharper contrasts developing? Or, is the tendency one toward *quantitative uniformity*" (the italics are mine)[2] Consequently, it is apparent that the use of the arithmetic average from which to measure dispersion follows the current conception that any tendency away from equality in distribution means greater concentration, the economists' term, or greater variability, the biologists' term.

"Mr. Persons gives no reason for using the standard deviation instead of the average error as a test of concentration." There is a very important and evident reason which I did not feel called upon to state. The shape of the area A in the diagram on p. 434 of my paper[3] does not affect the average error $\frac{\Sigma 1 x 1}{n}$. In other words, there could be a change in distribution among those above the average or among those below the average with absolutely no change in the average error. It is only by the use of some system that gives greater weight to the estates more distant from the average on either side (having larger deviations) than to those less distant (having smaller deviations) that the *shape* of the curve picturing estates according to size can influence the numerical size of the sum of the deviations. The merit of the coefficient of variability is that it is sensitive to any change that is likely to occur.

[1] Quarterly Journal of Economics, Vol. xix, p. 342.

[2] Publications of the American Economic Association, Vol. viii, No 4, p.161.

[3] Quarterly Journal of Economics, Vol. xxiii, May, 1909.

III

Dr. Watkins says that the coefficient of variability "varies extraordinarily for the same figures, yet this suggests nothing to him (Mr. Persons) as regards the limitations of his test." On page 439 of the article that Dr. Watkins is criticising I have said, "It is to be noticed that the coefficient of variability decreases as the range of incomes is made less. On the other hand, α of Pareto's equation does not necessarily decrease. It is not legitimate to compare the concentration of different ranges of income by means of the coefficient of variability, but it is legitimate to compare α's for different ranges, if α be considered a good measure of variability. By the very process of computing α, *the law of distribution over the entire range is assumed.*" (Italics do not appear in the original.) If the coefficient of variability is computed for incomes over \$5000, the resulting coefficient measures the variability, not of all incomes, but merely of those over \$5000. It is incorrect to assume, as Dr. Watkins does in the application of his logarithmic test, that the same distribution holds for the incomes below \$5000, or for all incomes, that holds for incomes above the \$5000 limit. In comparing the coefficients based upon the statistics of two periods only those estates above equivalent inferior limits should be in the computation. If Dr. Watkins's results are not affected by his not having complete data, there must be something wrong about his method. All that any method can do is to interpret the figures before us.

If the per capita wealth of the second of two periods compared be greater than the per capita wealth of the first of these periods it would be desirable to compare concentration for estates above the same *relative*

inferior limits. The attempt to make such a comparison leads to two difficulties which seem insurmountable. The first is to ascertain the relative value of equal fortunes in the two periods, and second, having done so, to get statistics complete enough so that the relative value can be used. These difficulties are not peculiar to the application of any one method, but to all methods of measuring concentration.

That the application of the coefficient of variability to estates above an inferior limit (called " truncation " by Dr. Watkins) is legitimate was demonstrated on page 435 of my article. A simple numerical illustration may make the point clear. Suppose that six individuals have wealth proportionate to the numbers 1, 2, 3, 4, 8, and 12.

The coefficient of variability for the six estates		=188
" " " " " "	first three estates	= 71
" " " " " "	last " "	= 71

The first coefficient is greatest because the range is greatest. The second and third coefficients are equal because the distribution among each set of three estates is the same. Since four times each of the first three estates respectively gives the last three, it is evident that the concentration *should* be the same for the two sections.

IV

Dr. Watkins claims that there is no advantage in having a numerical measure of concentration and that the net result in comparing his logarithmic curves " is clearly indicated by the total divergence of the curves." If he means by " total divergence of the curves " the distance between the upper extremities of the curves minus the distance between the lower extremities of the same curves, then he obtains a

numerical measure which neglects the intermediate distribution. Between the extremities the curves may be convex or concave. The decided divergence of Watkins's curves from a straight line may be seen by sighting along the ones given in Diagram III. Pareto avoids the error of neglecting part of the data by finding the straight line which best fits the points located by the statistics. Straight lines have the same slope throughout and hence it is possible to compare the numbers representing their slopes. If the points determined by the statistics do not follow a straight line then the closest fitting parabola may be found. Pareto's method is valuable so long as the " fit " is close, but it was shown (on pp. 425–27) that the error is considerable even in the case most favorable to the application of the method, namely, Prussian incomes.

Dr. Watkins thinks that the use of a numerical measure would lead us to draw such erroneous conclusions as this: " concentration for all successions in France is over twice as great (1620 is the coefficient) as those of 1000 francs (709), and this again over twice as great as for those over 250,000 francs (279)." Can one tell *subjectively* when one stone is twice as hard as another, when one hill is twice as steep as another, etc.? The ratio between two measurements is merely numerical and depends upon the measuring system that we adopt. However, it is not worth while to quarrel about the relative merits of numerical and graphical measures. Both are useful. Dr. Lorenz's method, when scales are used that enable one to follow the relative positions of the two curves, tells what happens all along the line as well as can be pictured to the eye; while the coefficient of variability gives the net result.

V

Dr. Watkins says that I draw inconsistent conclusions from the application to the Massachusetts Probate Returns for 1829–31 and 1889–91, of Dr. Lorenz's method and the coefficient of variability. On page 417 I say " The curve for the period 1829–31 shows

TABLE

Showing the Coefficients of Variability for Massachusetts Probates of 1829–31 and 1889–91

	1829–31	1889–91
All Estates	510	426
Estates of $1000 or over	366	366
Estates of $5,000 or over	242	252
Estates of $25,000 or over	133	157
Estates of $1000,000 or over	67	78

The greater the coefficient the greater the inequality in distribution for the class of estates taken. As the range decreases the coefficient decreases. Coefficients for estates above the same inferior limit are to be compared in order to determine the tendency in wealth distribution between the two periods for the estates above the inferior limit taken.

ESTIMATES

Taken from the Lorenz Curves for Massachusetts Probates of 1829–31 and 1889–91

The curve for 1829–31.

Shows greater bulge for approximately the poorest 80% of the estates owning approximately 20% of the wealth.

Shows less bulge for the next 19% of the estates owning some 45% of the wealth.

Shows greater bulge for the richest 1% of the estates owning some 35% of the wealth.

(Greater bulge indicates greater concentration).

the least bulge, except at the lower extremity, where it shows next to the greatest. — All of the curves are nearly coincident for the lower 50 per cent of the estates and for the most concentrated 40 per cent of the wealth. The method is defective in not allowing comparison at these points." It is because of the impossibility of comparing conditions at the upper and lower extremities, and because the curves cross that one concludes that "according to this method of measurement, the least concentration exists among these estates." I intended to caution the reader by the above statements that further analysis was necessary before a legitimate conclusion could be drawn. Such analysis showed the consistency of the two methods. The accompanying estimates are taken from the Lorenz curves based upon all the Massachusetts Probates of 1829–31 and 1889–91. The estimates are not based upon the chart given on page 417 of my article because, as I have explained, the scale is not such as to show what happens at the extremities of the curves. The two curves have been carefully redrawn in two sections, first, to show the lower ends and, second, to show the upper ends of the curves. The estimates are taken from the redrawn curves. The coefficients of variability for various classes of the same data are given in the table above. The coefficients 510 and 426 show that the concentration *among all estates* was greater in 1829 than in 1889. This conclusion is consistent with the Lorenz charts based upon exactly the same data. The concentration among estates above $5,000, is greater in 1889 than in 1829, which is also the result at which Dr. Watkins arrives. However, the exclusion of estates below $5,000, throws out 85.7% of the estates in 1829, and 69.6% in 1889. Why should such a large portion

of the data be eliminated? We are making comparison between two sets of data collected in the same way.

The upper portion of the Lorenz curve for 1829 shows a greater bulge than that for 1889. The coefficients of variability for estates of $100,000 or over show greater concentration in 1889. These results are not inconsistent because the Lorenz curves are based upon *all* the data while the coefficients are based upon the meagre statistics of estates above $100,000.

In criticising the Lorenz method Dr. Watkins says that the process calls for complete data and that " what is needed is a test of concentration that is applicable to incomplete data." Both the Lorenz method and the coefficient of variability are applicable to any of the data to which Dr. Watkins has applied his logarithmic method. Dr. Watkins seems to think that the application of the Lorenz method to estates of over (say) £100, gives no result of value. By drawing the curves for such estates for two periods on a scale that will enable a comparison of the extremities, a valid conclusion as to the relative concentration *among those estates* can be drawn, unless the curves cross. In case the curves do cross the coefficient of variability must be computed for the same estates in order to tell the net result. Does Dr. Watkins really think that any method will enable us to get an accurate statement of relative concentration for the *entire* population in two periods where " the line drawn between the propertyless and the small-propertied is bound to be very uncertain and unreliable? " By comparing the slopes of the logarithmic curves for but a portion of their course Dr. Watkins omits the smaller estates without knowing it. Then too, as has been previously pointed out, the two logarithmic curves

vary in curvature throughout their entire length and it is mathematically wrong to speak of a comparison of curvature, unless one means the curvature at two definite points, one point on each curve. By finding the slope of the best fitting straight line, Pareto determined what might be called the average curvature. It would be legitimate to use this average curvature in comparing slopes if the straight lines really picture the distribution of estates with but slight error. Analysis has shown, however, that the error is considerable, too considerable to depend upon the accuracy of the test.

VI AND VII

Does Dr. Watkins appreciate the mathematics at the basis of his logarithmic test and the reasons that lead Pareto to adopt as a measure of variability the slope of the straight line best fitting the points determined by the logarithms of the income data? In the first place Dr. Watkins gives "the straightness of the logarithmic curve" as a reason for connecting *adjacent* points by straight lines. Pareto has applied the legitimate method by taking advantage of the approximate straightness of the logarithmic curve to determine the *single* straight line which best fits *all* the points. The supposed adaptability of the logarithmic test is due, first, to the graphic use of logarithms which makes the errors of the "fit" appear to be less than they really are, and second, to the omission of a large portion of the statistics in getting the net difference of slant of the two logarithmic curves. Thus in comparing the slopes of the two curves in Diagram III, Dr. Watkins is compelled to base his conclusions upon but two pairs of points, for he says, "At the 100,000 franc point the two curves are $12\frac{1}{2}$ small squares apart;

near the upper ends, 17." Neither the curvature between these points nor below 100,000 francs (and hence the distribution) is taken account of. The coefficient of variability, on the other hand is influenced by all of the available data.

Even tho we had a generally accepted method of measuring concentration, there are not sufficient statistical data for measuring it for the United States, European data are but meagre, and no *certain* conclusion as to relative concentration can be drawn from the French and English statistics because *we are not sure that the data are comparable.* However, I have not found any evidence leading me to doubt the accuracy of the test that I recommend — the coefficient of variability. The coefficient and the Lorenz test are applicable to any of the data that Dr. Watkins has used and give consistent results in every case examined. Yet, since the two tests are not identical, it is probable that statistics can be "fixed up" to give different results, but there would have to be very slight difference in distribution between the two sets of statistics compared.

There is need for a method of interpreting wealth statistics that does not allow the subjective element to enter. The coefficient of variability is such a measure. It does not tell the whole story concerning the distribution of wealth, but simply enables one to answer the single question, is the concentration among one set of estates at one period greater or less than the concentration among another set of estates at another period?

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